Review report

Preprint egusphere-2025-2671-manuscript-version3

Title: Analytical approaches for wave energy dissipation induced by wave-generated turbulence and random wave-breaking

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Overall assessment

The authors have revised the manuscript by adding two appendices that supplement the derivation process in Section 2. I now believe that the basic assumptions and the logical steps of the derivations are clarified, and readers should be able to evaluate the credibility of the theory.

That said, I still have doubts about some of the assumptions underlying the formulations. For example, in Appendix B, the authors estimate turbulent mixing through scaling with water-particle displacement and orbital velocity of linear wave solutions. This is questionable from a physical standpoint, since purely oscillatory motions do not lead to irreversible mixing. However, I understand that fully justifying the physical validity of these assumptions may go beyond the scope of the present study, and the paper may be published as long as a rigorous comparison against reliable data is provided.

Regarding the comparison of the model against the tank experiments (L320-354), they try to rebut my previous comment that points out the problems in TKE budget framework. Specifically, they argue that wave breaking, wind-driven turbulence, and Langmuir turbulence do not contribute significantly to water turbulence, in a 16 m-long wind-wave flume under 20 to 30 m/s wind forcing, compared with non-breaking wave-induced turbulence. I find this argument physically implausible and not adequately supported. Under such strong wind forcing and limited fetch, it is difficult to accept that these well-established turbulence-generating mechanisms would be negligible.

Given the weak physical basis of the analysis in L320-354 and the fact that the comparison in L419-451 remains inconclusive due to the large scatter of the reference data, I consider that the proposed model is still not sufficiently validated against realistic observational or experimental evidence, nor supported by a rigorous theoretical foundation. Unless the theoretical arguments underlying the analysis in L320-354 are substantially improved, I cannot recommend the manuscript for publication.

Major comments

1. Response 1.2.5

The authors argue that TKE production by breaking waves, wind-driven currents, and Langmuir turbulence is negligible. This claim deviates substantially from the common understanding of wind-wave dynamics and mixed-layer turbulence: in young sea, waves actively break and enhance turbulence; such a situation is also commonly observed in wind-wave tanks. Such a departure would require particularly strong and convincing justification, not in the Response to Reviewer Comments but in the manuscript itself. However, the theoretical arguments presented by the authors are weak and rely on crude reasoning, as pointed out below. Given possibility of other turbulence source as discussed below, it seems unreasonable to assume ε_{dis} (TKE dissipation in water) $\approx e_{tid}$ (wave energy loss due to nonbreaking wave-induced turbulence).

- *"... the measurements were recorded for nonbreaking waves, and the wave steepness in Table 1 is less than the classical breaking criterion 1/7, so the breaking effects can in fact be neglected here."*

  Even in wave textbooks, it is acknowledged that the criterion $H/\lambda \approx 1/7$ is the limit of steepness of monochromatic Stokes wave and should be considered as an upper limit of individual wave steepness. In irregular wave fields, waves break at much smaller steepness (e.g., Figure 6 of Holthuijsen and Herbers, 1986, JPO), and in typical sea states H_s/λ_p is well below 1/7, yet breaking waves are widely recognized as a major sink of wave energy. Do the authors then suggest that wave breaking is also insignificant in realistic sea states?

- *"The wave-induced mixing strength is much larger than that calculated from the Mellor-Yamada turbulence closure scheme in the upper layers (e.g. Qiao et al., 2010; Xia,2015). The latter can also be neglected here in the laboratory experiments, for the wind wave tank is only 16 m long and the near-surface wind-driven turbulence is still in its incipient stage due to the short fetch."*

  The logic of this argument appears circular: wind-driven turbulence is assumed to be negligible precisely because nonbreaking wave-induced mixing is presumed to be dominant. However, as discussed in the Overall Assessment, the physical basis for estimating nonbreaking wave-induced mixing itself is questionable. Consequently, the claim that wave-induced mixing dominates over wind-driven turbulence is also not convincing.

  Furthermore, I do not see a clear justification for assuming that wind effects are also weak in the laboratory experiment. A limited fetch does not necessarily reduce the wind stress or the resulting shear-driven turbulence in the water. Rather, it is the wave growth that is constrained by the short fetch, which directly limits wave-induced mixing. This consideration further undermines the assumption that wind-driven turbulence can be neglected under the experimental conditions considered.

- Comparison against Langmuir turbulence (LT)
  I am unclear about how the scaling for Langmuir turbulence (LT) is chosen. If there are existing studies that support the adopted scaling, the authors should explicitly cite them. It appears inappropriate to characterize LT using the orbital-motion-induced displacement ($\bar{l}_D$), since LT is generally associated with mixed-layer-scale circulations. Furthermore, it is common to characterize the velocity scale with $(u_*^2 u_S)^{1/3}$, where u_* is water-side friction velocity, on which the present scaling does not depend (see, for example, Grant and Belcher, 2009, JPO).